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Hume Studies Volume XXII, Number 2 (November, 1996) 211-230.

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Hume's Scepticism with Regard to Reason: A Reconsideration

FRANCIS W. DAUER

The significance of Book I, Part iv, Section 1 of the *Treatise* may well be the naturalistic conclusion Hume draws from his skeptical argument.¹ Still, the conclusion is based on the argument, and the tide of opinion seems to be against the argument despite William Morris's valiant attempt to defend it.² Hume's argument certainly seems fishy and the conclusion that reliance on reason would lead to the total suspense of judgment is clearly unappealing. Unfortunately, I can't share the critics' confidence that Hume's argument is mistaken. I think a reasonably systematic attempt to understand Hume's argument will show that the critics haven't clearly won the day.

1.

Hume's skeptical argument makes two points: (a) all knowledge degenerates to probability, and (b) if we follow the dictates of reason, all probabilities reduce to nothing. Without too much injustice, I think we can take (a) to claim that all *a priori* beliefs are attended with some doubt, and take (b) to claim that if we rely on reason, we can have no confidence in a belief that is attended with doubt. I shall mainly be concerned with (b), but perhaps a few words about (a) are in order.

Concerning (a), Hume's central argument seems to be this:

Now as none will maintain, that our assurance in a long numeration exceeds probability, I may safely affirm, that there scarce is any

proposition concerning numbers, of which we can have a fuller security. For 'tis easily possible...to reduce the longest series of addition to the most simple question.... But knowledge and probability...must be either entirely present, or entirely absent.... if any single addition were certain, every one woul'd be so, and consequently the whole or total sum. (T 181)

In fact, any complex sum can be reduced to a series of simple +1 and -1 operations: $55,438+12,134 = [55,438+1]+[12,134 -1] = 55,439+12,13 = \dots 67,572+0 = 67,572$. Hume's point is: Because the complex sum can be mistaken only if some of the simple steps are, we shouldn't have doubts about the complex sum if there were no doubts about the simple steps. Given our doubts about the complex sum (because of past mistakes in similar cases), the *a priori* beliefs corresponding to the simple steps are also dubitable (because we must have been mistaken in similar cases in the past).

The obvious response is along the following line: "In my mind I recognize well enough what adding 1 or subtracting 1 is; but in writing this down in a calculation, I mistranscribe my belief. Thus, while I may write '55,437' for '55,438+1', in my mind I clearly saw that $55,438+1 = 55,439$." Much in the same region of thought is Imlay's suggestion:

Hume himself...has...insisted on the inconstancy of our mental powers. But, surely, such inconstancy would allow me to add 7 and 5, say, get 12, forget and put down 13 instead.³

Granting that slips of the pen or memory can happen, it's dubious that our errors are always of this sort. First, it's far from clear that there is always a separate act of the mind that recognizes the correct sum so that the error is only a faulty transcription or recollection. Isn't it more likely that in most cases I do the addition in the writing? But then, even simple additions are fallible. Secondly, even if there is a separate act of the mind, surely I believe that what I wrote down is correct—for example, that '55,438+1 = 55,437' is true. If we allow disquotation, I did mistakenly believe that $55,438+1 = 55,437$. Though disquotation requires care, given that I perceive the inkmarks whose meanings I know, what could prevent it? It would beg the question and be arbitrary to insist that disquotation must not result in simple *a priori* errors. And whatever care disquotation may require me to take, it won't suffice to prevent simple *a priori* errors—after all, no amount of care at each step can guarantee the complex sum to be correct.⁴ Thirdly, if the "errors" leading to the faulty complex sum are only mistranscriptions of infallible beliefs, how can I be mistaken about the complex sum? My only error seems to be falsely believing the calculation to involve no errors of transcription or memory. Complex and simple sums seem to hang together, and once it's granted that

the former can be mistaken, it's difficult to deny this for the latter.

With this said in favor of (a), let's turn to our main concern (b), where Hume claims all probabilities reduce to nothing:

In every judgment, which we can form concerning probability, as well as concerning knowledge, we ought always to correct the first judgment, deriv'd from the nature of the object, by another judgment, deriv'd from the nature of the understanding. (T 181-182)

Having thus found in every probability, beside the original uncertainty inherent in the subject, a new uncertainty deriv'd from the weakness of that faculty, which judges, and having adjusted these two together, we are oblig'd by our reason to add new doubt deriv'd from the possibility of error in the estimation we make of the truth and fidelity of our faculties. ...But this decision, tho' it shou'd be favourable to our preceding judgment, being founded only on probability, must weaken still further our first evidence...and so on *in infinitum*; till at last there remain nothing of the original probability. (T 182)

A central unclarity is what meaning or meanings we are to assign to Hume's use of 'probability'. Let us initially leave this relatively indeterminate and assume only that Hume's probabilities range between 0 and 1 so that a judgment with a higher probability has some virtue like a greater chance of truth or a higher degree of confidence or justification.

Hume's argument evidently starts with judgments like (a) $1+1 = 2$, (b) I shall eventually die, and (c) the next two tosses of this coin will have at least one heads. When we rely on the nature of the objects, even with a loose sense of 'probability', the probability of (a) and (b) seems to be 1 and that of (c) something less (perhaps $3/4$ if we rely on frequencies in similar coin tosses). The initial situation thus involves accepting a proposition o with some degree of confidence where this confidence is the concomitant assessment of o 's probability based on the nature of the object. Letting $P_1(o)$ be the assessed probability of the accepted proposition, the initial situation may be represented as one where $P_1(o)=x_o$. If o is (a) or (b), we supposed $x_o=1$; if o is (c), x_o might be $3/4$. We shouldn't assume that $P_1(o)$ is a probability in any mathematical sense. For example, if "justified degrees of confidence" should best interpret Hume's talk of probabilities, "probabilities" of this kind may not obey the laws of mathematical probabilities. I use 'P()' for Hume's so far indeterminate use of 'probability' and reserve the notation 'Pr()' for probabilities satisfying the mathematical theory of probabilities.

According to Hume, since the first assessment of o 's probability relied on our fallible faculties, we must assess their reliability. Suppose that R_1 is the

calculation or reasoning (based on the nature of the objects) that led us to assign x_0 as o 's probability. Let r_1 be: The reasoning or calculation R_1 is OK. I use the loose 'is OK' since it is not initially clear what kind of success or failure of our faculties we are supposed to assess. Despite this looseness, since our faculties aren't infallible, it seems plausible that the probability we can assign to r_1 is less than 1, that is, $P_1(r_1) = x_1$, $x_1 < 1$.

Hume's next step is: Since our first assessment of o 's probability relied on R_1 being OK, insofar as our faculties are fallible, we must readjust our assessment of o 's probability downwards from x_0 . Since we can't say $P_1(o) = x_0$ and $P_1(o) < x_0$, let's use ' $P_2(o)$ ' for the second or revised probability we assign to o . Hume's claim then is: Since $P(r_1) = x_1 < 1$, $P_2(o) = f(x_0, x_1) < x_0$. What this function f should be is unclear, but as long as $f(x_0, x_1) < x_0$ when $x_1 < 1$, no harm is done in taking f to be the product. Thus, without any essential loss of generality, $P_2(o) = x_0 * x_1$.

But this assessment relies on our reasoning or calculation R_2 which assigns x_1 to r_1 . Letting r_2 be the claim that R_2 is OK, r_2 's probability is x_2 , $x_2 < 1$. $P_2(o)$ must then be also revised downwards and the third assessment of o , $P_3(o)$, becomes: $P_2(o) * P_1(r_2) = x_0 * x_1 * x_2$. Thus, starting with o whose probability is first assessed to be x_0 (based on the nature of the objects), Hume's regress argument is:

Let r_i be: the reasoning or calculation R_i that assigned the probability x_{i-1} to r_{i-1} (or x_0 to o when $i=1$) is OK.

$$(A) P_1(r_i) = x_i, x_i < 1.$$

$$(B) P_{i+1}(o) = P_i(o) * P_1(r_i)$$

Using (A) and (B) repeatedly, as i increases, $P_{i+1}(o)$ clearly decreases. Thus, if we followed the dictates of reason, the probability or confidence we can assign to the accepted proposition o reduces to nothing.

Admittedly, the argument isn't quite sufficient for the skeptical conclusion. Fogelin noted that x_i might converge to 1; the probability losses would then converge to 0 and leave a finite probability for o .⁵ But, as he concedes, we couldn't be very comfortable if the viability of our beliefs depends on the reliability of our faculties converging to 1. It's also true that the probability doesn't actually reach 0. But as Morris pointed out, it suffices that the probability of o converges to 0: since "there is no non-arbitrary end to the stages of iterated assessments,"⁶ we can get as close to 0 as we like, that is, for any proposed (small) value D , the probability of o can be reduced to below D .

How then can we evade Hume's argument in a satisfying way? (A) is difficult to resist: our faculties are less than 100 percent reliable and we can't have 100 percent (justified) confidence in them. The best hope is to block (B), and two ways have been suggested by commentators: (I) Grant that our

faculties aren't fully reliable; but insist that this does not force any re-assessment of o 's probability. (II) Grant that o 's probability needs to be reassessed; but insist that (B) is incorrect and $P_2(o)$ needn't be less than x_0 . I shall suggest that neither way is entirely convincing.

2.

Fogelin for one has argued that $P_1(o)$ shouldn't be revised in light of our fallibility. Suppose that based on the nature of the objects we conclude $P_1(1+1 = 2) = 1$ and we consider our fallibility. Fogelin claims:

Hume's...point is that these considerations must lead us to lower the probability assignment given to the original proposition. This, however, is simply wrong. However certain or uncertain we are about our ability to calculate probabilities, if a proposition has a certain probability, that (tautologically) is the probability it has.⁷

Thus, the attempt to form the revised probability $P_2(1+1 = 2)$ is a mistake. But it seems to me clear that Morris has given the correct response:

If Hume were claiming what Fogelin has him saying here—that reflecting on our ability to assess probability changes the objective probability a proposition has—he would be wrong. But he is not claiming this. It is *my confidence* in having correctly assessed the probability that Hume claims should change. His regress is designed to further erode my confidence as the assessments iterate.⁸

Morris is surely right: if we take Hume's probabilities $P_i(o)$ to be in the region of degrees of confidence, there is no confusion in suggesting that probabilities in this sense can diminish while recognizing that the objective probability remains the same.

Morris's idea, however, requires further elaboration. Certainly Hume's central use of 'probability' in his discussion of probability of causes relates to degrees of confidence (or levels of force and vivacity). However, while $P_i(o)$ is not o 's objective probability, it also cannot be the degree of confidence in o after i -th consideration of the matter. Hume's ultimate view is that though one "can find no error in the foregoing [skeptical] arguments, yet...[one] continues to believe." (T 184) The erosion of probability is supposed to be what *would* happen if we *were* to follow the dictates of reason, and this is clearly Morris's point. Since our actual degree of confidence (or our subjective probability) does not erode (significantly), what erodes must be something else—the degree of confidence we can justifiably have, that is, the confidence we would have if we were to follow the dictates of reason. Let us then understand ' $P_i(o)$ ' as the degree of confidence we can justifiably have in o (on the i -th

consideration of the matter). If we use the term 'credibility' for this justified degree of confidence, on this understanding of Hume's use of 'probability', what erodes is the credibility rather the subjective or objective probability.

This, however, raises the question of how we should understand justifiable degrees of confidence within Hume's philosophy. Since it relates to the dictates of reason, in the absence of specifying all the dictates of reason, it is unlikely that we can give a full account of credibility. However, some indications seem possible. Hume would certainly claim that if we recognize that our faculties err in about 10 percent of the cases, full confidence in the judgments of our faculties would not be justified. More contentiously, I would suggest that *at least for the sake of the argument*, he would also be willing to grant that *in the absence of other dictates of reason*, we would be justified in having 90 percent confidence in the judgments of our faculties. After all, his point here is not to reintroduce his inductive skepticism. Generalizing from our example, we might say: in the absence of other dictates of reason, the justified level of confidence one can have in p is the degree to which one's evidence supports p .⁹ The degree to which one's evidence supports p would seem to be the objective probability of p indicated by one's evidence (where for Hume this would largely be a matter of projecting the observed frequencies). Such an account seems to be at least in keeping with the passage (with my italics) where Hume distinguishes between knowledge, proof, and probability:

in common discourse we readily affirm, that many arguments from causation exceed probability, and may be receiv'd as *a superior kind of evidence*... 'twould perhaps be more convenient, in order...to...mark the several *degrees of evidence*, to distinguish human reason into [knowledge, proofs, and probabilities]...By probability, [I mean] *that evidence*, which is still attended with uncertainty. (T 124)

Given these suggestions, the credibility of o on the first consideration of the matter is the objective probability of o that is indicated by one's evidence. That is, when $P_1(o) = x_0$, the credibility x_0 that o has for me is given by my reasoning R_1 (based on the nature of the objects) which assesses the objective probability $\text{Pr}(o)$ to be x_0 . This appears to be what Morris is claiming when he says, "It is *my confidence* in having correctly assessed the probability that Hume claims should change." The confidence that *should* change is o 's credibility while the assessed probability is our (possibly mistaken) initial assessment of o 's objective probability. The full answer to Fogelin then becomes this: While the initial credibility o has for me is my assessment of o 's objective probability, as other dictates of reason come into play, the justified degree of confidence I can have in o reduces. This is not to say that the objective probability of o (or even our assessment of o 's objective probability) reduces.

While Fogelin's objection can be met in this manner, a somewhat related objection could prevent Hume's argument from getting started. If my initial assessment of *o*'s objective probability is x_0 , even if serious doubts exist about my assessment, as long as my assessment is in fact correct, wouldn't x_0 be the justified degree of confidence I can place in *o*? In fact, if the justified degree of confidence is the objective probability indicated by my evidence, even if I form *no* assessment of *o*'s objective probability, wouldn't my level of confidence x_0 in *o* be justified if x_0 is the objective probability indicated by (or projectible from) my evidence?

For Hume, the answer would of course be negative because other dictates of reason must be brought into play, and we can well expect one of them to be: I am justified in having a level of confidence x_0 in *o* only if I assess *o*'s objective probability and can be justified in having confidence in this assessment being OK. In short, Hume's argument is conducted within an internalist framework. As such it could be blocked by an externalist who claims that one's assessments of objective probabilities as well as one's levels of confidence are justified as long as they are engendered by a cognitive mechanism which is reliably connected to the objective probabilities of the world. Since the externalist outlook insists that our being so justified does not depend on our being able to justify or know the mechanism to be reliable, Hume's argument can't get started within such an outlook. Whether or not the externalist outlook is viable, when Hume attacks reason, he is pretty clearly attacking the role claimed for it by internalists like Descartes. Beyond this, there may not be too great a distance between externalism and Hume's rejection of reason in favor of Nature which determines us "to judge as well as to breathe" (T 183) and happily breaks the force of all skeptical arguments (T 187). Thus, I suggest Hume's argument be seen as working within an internalist framework having dictates of reason of the sort suggested.

However, even within an internalist outlook, Ian Hacking argues that adjusting one's initial degree of confidence because of our fallibility confuses two levels of probability:

Hume regularly confuses what we may call levels of probability. A probability of the first level arises from the evidence bearing directly on the case at issue. Second level probabilities concern the extent to which one can rely on inferences to probabilities at the first level.¹⁰

Hacking's idea may be exemplified by a variant of his example: Having found 6 cars to be domestic and 4 to be imports, I reason that the probability of the next (randomly chosen) car being domestic is 0.6. Suppose I now examine 1,000 cars and find 600 to be domestic. My epistemic situation has improved and I am now more justified in relying on my probabilistic inference—the "second" level of probability has increased. But the assessed probability for the

next car being domestic—the “first” level of probability—remains unchanged at 0.6. Hacking draws the moral that the two levels of probability must be kept distinct. If we take credibilities or justified degrees of confidence to be direct projections of the assessed objective probabilities, the credibility of the next car being domestic would also be unaffected by considerations concerning the reliability of our faculties or our reasoning. Thus, Hacking says: “Had he [Hume] spoken of...different levels of probability he would more easily have explained the failure of skepticism with regard to reason,”¹¹ that is, more easily than in terms of nature preventing reason from having much force.

Hacking is clearly right about our assessment of objective probabilities: lowering the assessed probability of the next car being domestic when only 10 cars were observed (or increasing it when 1,000 were observed) would over- (or under-) estimate the probability of its being an import. I think this does show that even if our assessment of *o*'s objective probability is recognized to be fallible, this should not lead us to reduce the assessed objective probability of *o* (let alone *o*'s objective probability). However, it's less clear that credibilities must always be direct projections of the assessed objective probabilities our epistemic situation allows. After all, there are other dictates of reason. Couldn't there then be a “composite” degree of justified confidence based on the assessed objective probability and the reliability of the assessment? The initial credibility $P_1(o)$ may well be a direct projection of *o*'s assessed objective probability; the credibility $P_1(r_1)$ may also be a direct projection of the assessed objective probability of our faculties being OK. But couldn't we try to develop a scheme for assigning a revised composite credibility $P_2(o)$ based on the two levels of credibility $P_1(o)$ and $P_1(r_1)$? Developing such a scheme may be difficult and composite credibilities may not turn out to be mathematical probabilities, but must there be an error in even trying?¹²

If it is an error for doubts about our faculties to affect our initial justified confidence, the so-called Preface Paradox wouldn't be paradoxical. In a preface an author writes, “Given the limitations of human reason, despite my best efforts, I'm sure some of my claims are mistaken.” Let ‘ q_1 ’, ... ‘ q_n ’ be the statements in the text proper, and take a statement to be warranted if its degree of justification (or the justified degree of confidence one can have in it) is enough for knowledge. In the text the author implicitly claims *T*, and in the preface, *P*:

- T*: ‘ q_1 ’, ... ‘ q_n ’ are all warranted.
P: “‘ q_1 & ... & q_n ’ is false’ is warranted.

Since an inconsistent set of statements can't be warranted, the author's statements are paradoxical because *P* seems to give: ‘ $\sim(q_1 \& \dots \& q_n)$ ’ is warranted. If Hacking's two levels of justified confidence must be kept distinct, *T* is about warrant₁, *P* about warrant₂, and the paradox vanishes.

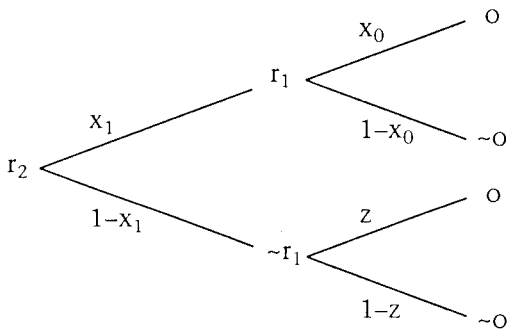
While the paradox can be so defused, it is far from clear it *should* be defused. There is a strong intuition that the author is after all involved in some sort of an inconsistency in claiming both T and P. Either the author should have refrained from P or else we should see P as a retraction of the (implicit) claim that *all* his statements in the text are warranted. In the latter case, the degree of justification initially claimed for the statements in the text must be adjusted downwards, and doubts about the reliability of our faculties *do* affect the justified degree of confidence we initially claimed. Since there seems to be no decisive reason that the paradox should be defused, there is no clear mistake in trying to devise a scheme where o's initial credibility $P_1(o)$ is adjusted to $P_2(o)$ in light of our fallibility whereby $Pr_1(r_1) < 1$.

3.

A fairly obvious scheme for developing the needed "composite" credibility or justified degree of confidence is along the following lines: Initially by reasoning R_1 on the nature of the objects, we assess the probability of o to be x_0 in an objective or frequency sense. Interpreting Section 1's 'is OK' in terms of having given the correct result, we can say:

- (1) Let r_i be: The correct probability was assigned by the reasoning R_1 that assigned probability x_{i-1} to r_{i-1} (or x_0 to o when $i=1$).

The initial credibility we attach to o occurs in the epistemic circumstance where we unquestioningly accept that r_1 . This credibility, that is, $P_1(o)$, can be taken to be $Pr(o|r_1) = x_0$, where 'Pr()' is a genuine probability. The tree diagram below will be helpful. On it, each branch assigns the conditional probability of the next node on the path given the truth of all the nodes on that path up



to that branch. The sub-tree starting with r_1 represents the initial situation where I unquestioningly accept r_1 .

Given that our faculties are fallible, our reasoning R_2 determines the objective probability of r_1 to be x_1 , $x_1 < 1$. Our epistemic circumstance has now

changed: while we unquestioningly accept r_2 (the correctness of R_2), we no longer accept r_1 unquestioningly; and our revised situation is represented by the entire tree diagram shown. The revised "composite" credibility $P_2(o)$ and the initial credibility of r_1 that isn't unquestioningly accepted should be understood as $\Pr(o|r_2)$ and $\Pr(r_1|r_2)$. In general the i -th assessment of o 's credibility and the first (reasoned) assessment of r_{i-1} 's credibility occur when we accept r_i unquestioningly. Thus, an interpretation of ' $P_i(o)$ ' is given by:

$$(2) \quad \text{a. } P_1(r_i) = \Pr(r_i|r_{i+1}), \quad \text{b. } P_i(o) = \Pr(o|r_i).$$

Let I_1 be the interpretation given by (1) and (2).¹³ Under I_1 , Hume's (A) from Section 1 [i.e., $P_1(r_i) = x_i, x_i < 1$] becomes:

$$(A_1) \Pr(r_i|r_{i+1}) = x_i, x_i < 1.$$

(A_1) is hard to resist. As long as our reasoning R_{i+1} takes our fallibility into account, it should assign r_i a probability $x_i, x_i < 1$. On the condition that R_{i+1} assigned the correct probability, the (conditional) probability of r_i is trivially $x_i, x_i < 1$. And that's what (A_1) claims. As for Hume's (B) from Section I [i.e., $P_{i+1}(o) = P_i(o) * P_1(r_i)$], under I_1 it becomes:

$$(B_1) \Pr(o|r_{i+1}) = \Pr(o|r_i) * \Pr(r_i|r_{i+1}).$$

If (B_1) is correct, since (A_1) assures the second product in (B_1) being less than 1, $\Pr(o|r_{i+1})$, and hence $P_{i+1}(o)$, decrease as i increases. Hume's argument sketched in Section 1 would thus be sustained.

However, adjusting Imlay's remark to interpretation I_1 , he objects to (B_1) by noting that the diminution of credibility claimed by Hume

would inevitably occur if in order to find the probability of a probability I simply multiplied the two together. Such a procedure, however, ignores the probability however slight that the original probability [viz., the probability of o] is different and perhaps even higher than I initially judged it to be.¹⁴

Our tree diagram makes Imlay's point clear: Hume's (B_1) considers only the top-most path and neglects the path that leads to o via $\sim r_1$. Since nothing Hume says prevents the values $x_0 = .8, x_1 = .7$, and $z = .9$, Imlay claims:

If...I add the two [paths] together, as I should, I get $.7 \times .8 + .3 \times .9 = .83$. This number, needless to say, is higher than the one [.8] assigned as the original probability of [o].¹⁵

Imlay's objection is based on Reichenbach's response to C. I. Lewis's views on certainty.¹⁶ Applied to our case, Reichenbach's claim becomes:

$$\Pr(o|r_2) = \Pr(o|r_1)\Pr(r_1|r_2) + \Pr(o|\sim r_1)\Pr(\sim r_1|r_2) = x_0 * x_1 + z*(1-x_1)$$

This is a theorem of the probability calculus and our tree diagram gives it intuitive sense. Let the path probability be the product of all the branch probabilities on the path. The theorem states that the conditional probability of *o* given r_2 is the sum of the path probabilities of all paths from r_2 to *o*. Under I_1 Hume's argument fails and Imlay is clearly right: (B_1) is mistaken by neglecting all paths to *o* except the top-most one.¹⁷

Karlsson objects to Hume along similar lines: Asked whether his current confidence isn't reduced by past errors in probable reasoning that led him to place wrong degrees of confidence in judgments, Karlsson replies:

...no: "My dear sir," say I, "I rarely err, and when I do, it is as often in placing too little confidence in my judgment as in placing too much. This being so, your observation does not lead me in any wise to reduce the confidence I place in my judgment."¹⁸

Karlsson in effect divides the path on our diagram from $\sim r_1$ to *o* into two: (a) one path leading to *U* (for under-estimation) with probability 0.5 where *U* leads to *o* with probability x_0+D and (b) one leading to *O* (for over-estimation) with probability 0.5 where *O* leads to *o* with probability x_0-D . Extending Reichenbach's formula to this case, $\Pr(o|r_2)$ becomes:

$$\Pr(o|r_1)\Pr(r_1|r_2) + [\Pr(o|U)\Pr(U|\sim r_1) + \Pr(o|O)\Pr(O|\sim r_1)]\Pr(\sim r_1|r_2) = x_0 * x_1 + [(x_0+D)(.5)+(x_0-D)(.5)](1-x_1) = x_0.$$

Hume's argument seems doomed under I_1 and his words tend to encourage I_1 when, for example, he says we

must enlarge our view to comprehend a kind of history of all the instances, wherein our understanding has deceiv'd us, compar'd with those, wherein its testimony was just and true. (T 180)

If our faculty is a kind of witness giving true or false testimony, r_1 becomes the first witness's testimony that the probability of *o* is x_0 ; r_2 becomes the second witness's testimony that there is an $1-x_1$ probability of the first witness having deceived us. To assess *o*'s probability from the second witness's perspective, one must take into account the probability *o* has if the first witness did deceive us. If the faculties are like witnesses, Hume can't limit himself to the top-most path on the tree diagram.

Against (B_1)'s dismissal along Reichenbach's lines, it's tempting to say: We are talking about *justifiable* degrees of confidence; when my basis for having confidence in *o* is r_1 , I can't claim any justifiable confidence resulting from the supposition that my basis r_1 is mistaken. But under I_1 this objection

comes to naught: At the *initial stage* one can't claim any justification for o derived from one's basis r_1 being mistaken. But the tree diagram for the initial stage reflects this: in the sub-tree starting with r_1 , no probability accruing to o is based on r_1 's falsity. At the *second stage* we base our assessment of o on r_2 which claims $1-x_1$ to be the probability of our initial reasoning having given the wrong result. At *this stage*, one's basis is no longer r_1 which claims the initial reasoning to have given the right result; our basis is r_2 and at this stage no probability accrues to o from r_2 being mistaken. In general, one's basis keeps shifting as the assessment of the faculties is iterated; at no point does Reichenbach's view credit one with any justifiable degree of confidence deriving from an error in one's basis *at that point*.

4.

I would rest content that Hume's argument is defeated were it not that another interpretation of it seems possible. Any mathematical notion of probability assumes that if q 's probability is x , $\sim q$'s is $1-x$. But is this always so for justified degrees of confidence? Let q state that God exists. Suppose the only reason I have for or against q is a proof for His existence; but I suspect my proof may be fallacious. Do I then have some justified degree of confidence for $\sim q$? Surely not. To think so would be something like the fallacy of the argument from ignorance. If I not only suspect but find out that my proof is no good, I have no reasons for q . But this doesn't give me any reasons for $\sim q$; to think otherwise is to commit the fallacy of the argument from ignorance. When I only suspect my proof, while my justified degree of confidence for q is less than 1 (say, 0.8), I still seem to lack reasons and any justified degree of confidence for $\sim q$.

Since justified degrees of confidence of the sort just suggested evidently aren't probabilities, let's introduce the notation ' $Cr(q)$ ' for it. Let $Cr(q)$ range between 0 (for having no reasons for q) and 1 (for being justified in having full confidence in q). Let $Cr(\sim q)$ also range in this way between 0 and 1. In our example concerning God's existence, we can expect $0 < Cr(q) < 1$ while $Cr(\sim q) = 0$. While credibilities of this sort clearly aren't probabilities, in some cases $Cr(q)$ can be more like probabilities in that $Cr(q) + Cr(\sim q) = 1$. In his discussion of the probability of causes Hume considers having found 19 out of 20 sea-going ships to return. Let q state that this sea-going ship will return. In conjunction with our earlier suggestion that (at least for the sake of the argument) the justified degree of confidence may *ceteris paribus* be taken to be a projection of the observed frequencies, Hume can be taken to suggest: $Cr(q) = 19/20 = .95$ and $Cr(\sim q) = 1/20 = .05$. The net confidence in q is evidently $.95 - .05 = .90$:

'tis evident, that as the contrary views are incompatible with each other, ... their influence becomes mutually destructive, and the mind

is determin'd to the superior only with that force, which remains after subtracting the inferior. (T 138)

Though not needed for our argument, we can understand the "net" credibility of q to be $Cr(q) - Cr(\neg q)$ when $Cr(q) > Cr(\neg q) > 0$.¹⁹

Given this background, a new interpretation might be proposed for Hume's argument. As in Section III, at the initial stage we assess by reasoning R_1 (based on the nature of the objects) that the objective probability of o is x_0 . 'R₁ is OK' should now be understood as R_1 contains no illegitimate steps, that is, each step of R_1 is justified or correctly inferred from previous steps. In general:

- (1) Let r_i be: The reasoning R_i that assigned (the objective) probability x_{i-1} to r_{i-1} (or x_0 to o when $i=1$) contains no illegitimate steps.

The initial justified degree of confidence or credibility we can assign to o occurs in an epistemic circumstance where we unquestioningly accept r_1 . Furthermore, since at this initial stage we can't do anything but accept r_1 , at this stage we would seem to have something like an epistemic right to do so. Let us then introduce conditional credibilities as follows:

- (2) Let $Cr(q|s)$ be the justified degree of confidence one can assign to q given that one can accept it.

In the initial circumstance, then, the credibility or justified degree of confidence we can assign to o is given by $Cr(o|r_1)$.

Continuing Hume's argument, once we reflect that we can't have full confidence in r_1 and assess the track record of our faculties, by reasoning R_2 we take the objective probability of r_1 to be x_1 . We can unquestioningly accept that R_2 contains no illegitimate steps because we can't do anything else at this point. And this becomes the basis for the first reasoned credibility of r_1 and the revised, second credibility of o . Extending these considerations, the interpretation of Hume's 'P₁()' becomes:

- (3) a. $P_1(r_i) = Cr(r_i|r_{i+1})$ b. $P_1(o) = Cr(o|r_i)$

Calling (1)-(3) interpretation I_2 , under it Hume's (A) [i.e., $P_1(r_i) = x_i, x_i < 1$] becomes:

- (A₂) $Cr(r_i|r_{i+1}) = Cr[r_i | \text{The reasoning } R_{i+1} \text{ that assigned the probability } x_i \text{ to } r_i \text{ contains no illegitimate steps}] = x_i, x_i < 1.$

Taking our fallibility into account, the probability our reasoning R_{i+1} assigns to r_i should be less than 1. On the condition that we can accept that R_{i+1} itself contains no illegitimate steps, the justified degree of confidence we can have in r_i is surely less than 1. So, (A₂) is again hard to resist. Under I_2 , Hume's (B) [$P_{i+1}(o) = P_i(o) * P_1(r_i)$] becomes:

$$(B_2) \text{Cr}(o|r_{i+1}) = \text{Cr}(o|r_i) * \text{Cr}(r_i|r_{i+1}).$$

To assess this, let $\text{Cr}(o|r_1) = x_0$ and $i=1$; (B₂) along with (A₂) then claim:

$$(b) \text{Cr}(o|r_2) = \text{Cr}(o|r_1) * \text{Cr}(r_1|r_2) = x_0 * x_1, \quad x_1 < 1.$$

To test (b)'s correctness, let's reinterpret the Section 3 tree diagram as a diagram for $\text{Cr}()$ with each branch giving conditional credibilities. The conditional credibilities x_0 and x_1 are unproblematic since they are dummy variables for whatever the conditional credibilities are. Furthermore, when we are assessing objective probabilities, whether this relates to objects or to our reasoning, the situation is like Hume's ship example; so, the conditional credibilities $1-x_0$ and $1-x_1$ should be all right. What values of $\text{Cr}(o|\sim r_1)$ and $\text{Cr}(\sim o|\sim r_1)$ should replace z and $1-z$? Consider

$\text{Cr}(o|\sim r_1) =$ The justified degree of confidence we can assign to o given that we can accept that the reasoning (that assigned the probability x_0 to o) contains illegitimate steps.

The assumption that the reasoning R_1 (which assigned the probability x_0 to o) contains illegitimate steps is much like the assumption that my proof for God's existence is fallacious, an assumption that leaves me with no reasons for or against His existence. Thus, it seems to me that $\text{Cr}(o|\sim r_1) = \text{Cr}(\sim o|\sim r_1) = 0$. Adjusting our tree diagram so that z and $1-z$ are both replaced by 0, the only justified degree of confidence accruing to o is from the top-most path.

The only remaining question is what credibility accrues to o from the top-most path. The two relevant situations are: (i) the situation where I can unquestioningly accept the legitimacy of the reasoning R_1 which assigns x_0 to o and (ii) the situation where (having no reasons for or against o besides R_1), I can and must accept that the probability of R_1 's legitimacy is x_1 , $x_1 < 1$. Surely in shifting from (i) to (ii) the justified degree of confidence in o must diminish. Once this is granted, no harm is done in taking $\text{Cr}(o|r_1) * \text{Cr}(r_1|r_2) = x_0 * x_1$ to be the credibility derived from the top-most path.²⁰ We have thus verified (b) since the top-most path is the sole contributor to $\text{Cr}(o|r_2)$. The argument can clearly be generalized to verify (B₂); hence, I_2 seems to sustain Hume's argument as sketched in Section 1.

Aside from the apparent success of Hume's argument, there are two

additional reasons for preferring I_2 over I_1 in interpreting Hume. First, recall how Hume introduced his argument: We need to correct the judgment “deriv’d from the nature of the object” by a second judgment “deriv’d from the nature of the understanding.” Under I_1 , the first instance of the second judgment would be: the reasoning assigning x_0 to o assigned the *correct* probability. This judgment is as much about the nature of the objects as the nature of the understanding, and if we take it to be mistaken, we can conclude that the objective probability of o isn’t x_0 . Also, when an utterly fallacious reasoning ends up assigning the correct probability, our reasoning wasn’t erroneous and shouldn’t weaken our confidence in our faculties. Under I_2 the first instance of the second judgment is: the reasoning assigning x_0 to o contains no illegitimate steps. This judgment makes no claims about the nature of the objects, and if we take it to be mistaken, our confidence in our faculties will be weakened but nothing can be concluded about the objective probability of o . It would be closer to Hume’s original division if uncertainties about the objects are kept distinct from uncertainties about the faculties.

Secondly, there is an obvious problem with I_1 : had Hume’s argument succeeded, o ’s (conditional) probability (which is supposed to measure our justified level of confidence) tends to 0. As Fogelin noted, this leads to

the surprising result that the negation of the original proposition must reach 1. Of course, reasoning in this way will quickly generate a paradox, for we can start over again and reduce the probability of the negated proposition to 0. Perhaps Hume would have enjoyed this ingenious paradox, but I see no hint of it in the text.²¹

Hume certainly can’t accept that $\Pr(\sim o|r_1)$ tends towards 1. But he can’t welcome the paradox either. Since it is a contradiction that $\Pr(o|r_1)$ and $\Pr(\sim o|r_1)$ both tend to 0, one of the premises leading to that result must be mistaken. As Hacking suggested, a plausible target would be the premise that any judgment must be adjusted by a judgment about the judgment’s reliability. Under I_2 , unless one commits the fallacy of the argument from ignorance, loss in o ’s credibility is not accompanied by a gain in $\sim o$ ’s credibility. As a result, if the argument is successful, the credibility of both o and $\sim o$ will tend to 0 with the result that we can have no justified degree of confidence in o and none in $\sim o$.

Despite the apparent success of Hume’s argument under I_2 and I_2 ’s overall preferability, caution suggests that we should assure ourselves that objections along Karlsson’s line cannot be raised again. Clearly Hume’s argument under I_2 will not be defeated by the mere truth of

k_0 : When r_1 is false (i.e., when reasoning R_1 is illegitimate), r_1 over-estimates o ’s probability as often as it underestimates it.

Unless I could have justified confidence that k_0 is true, no justified degree of confidence in o will accrue to me from whatever credibility I can attach to $\sim r_1$. Things would however be different if my epistemic situation contained a standing justification for statements like k_0 .

Letting D measure the amount of over- and under-estimation, let us then suppose that my epistemic situation contains a standing justification for the following assessment of objective (conditional) probabilities:

- k: $\Pr(o\text{'s objective probability is } x_0 + D \mid \sim r_1) = .4$ &
 $\Pr(o\text{'s objective probability is } x_0 - D \mid \sim r_1) = .4$ &
 $\Pr(o\text{'s objective probability is } x_0 \mid \sim r_1) = .2$.

From k it would seem I could reason that $\Pr(o \mid \sim r_1) = (.4)(x_0 + D) + (.4)(x_0 - D) + (.2)x_0 = x_0$. But then x_0 would seem to be the justified degree of confidence I can assign to o given that I can accept $\sim r_1$ (i.e., that the reasoning R_1 assigning x_0 to o contains an illegitimate step). However, this simply states that $\text{Cr}(o \mid \sim r_1) = x_0$. Thus, in our tree diagram the value z on the branch from $\sim r_1$ to o is x_0 , not 0 as claimed by (interpretation I_2 of) Hume. Hence, the justified degree of confidence I can have in o given that I can accept r_2 is $x_0 * x_1 + x_0 * (1 - x_1)$ or x_0 . While Karlsson may well not pursue such a line of thought, it would seem that Hume's argument won't be saved by I_2 if I have a standing justification for k .

However, I believe Hume can give some fairly plausible responses to this kind of an objection. If k is true, K must also be true:

- K: When we reason that o has a certain probability x_0 , it in fact has that probability whether or not we reasoned legitimately.

Granting that k (and hence K) could be true, the mere truth of k won't give rise to any degree of justified confidence in o . We need a justification or reasons for accepting k . But insofar as k entails K , k is surely a most surprising claim since it claims that reasoning illegitimately is no cause for alarm. Thus, even if k were true, it's difficult to see how we could reason to k within our framework of reasoning. Furthermore, even if our framework allowed us to reason to k , that reasoning must involve determining o 's probability (based on the nature of the objects). But how can we do this in a circumstance where we assume that R_1 , our first assessment of o based on the nature of the objects, is illegitimate or fallacious? Special circumstances aside, the general availability of a second direct assessment of o which isn't undermined by the assumed illegitimacy of the first assessment is most dubious. In sum, it would be difficult to urge that we could reason to k . But if we can't reason to k , the path leading to o through $\sim r_1$ can't give rise to any justified degree of confidence.

But suppose even that there is some reasoning which allowed us to accept k with some degree of confidence. It would still be wrong to think that on the

“credibility tree” we can simply take the branch from $\sim r_1$ to o to have the value x_0 . The justified degree of confidence I can assign to o on the path going through $\sim r_1$ depends on k and the justified degree of confidence I can have in k . If k is a dead certainty for me and I can have complete confidence in it, then even when I accept $\sim r_1$ (i.e., take R_1 to be fallacious), I can have x_0 degree of justified confidence in o , that is, the value of z on the branch from $\sim r_1$ to o is x_0 . But if the justified degree of confidence I can place in k is less than 1, since I’m relying on k , the value of the conditional credibility z will surely be less than x_0 . But then the second path to o won’t make up for the credibility loss suffered on the top-most path because when $z < x_0$, the amount one gains $(1-x_1)*z$ is less than $(1-x_1)*x_0$ and the latter is $x_0-x_0*x_1$ or the amount one loses. Surely Hume can plausibly claim that k can’t be a dead certainty for us. Given how implausible it was that we could have any reasons for k , surely no reasons are going to make k a dead certainty. In sum, Karlsson’s original objection cannot be reformulated in way that will prevent Hume’s skeptical conclusion under I_2 .

While it would be rash for me to conclude that we must accept Hume’s skeptical argument, I think that decent sense can be made of it and that it is not *clearly* mistaken. Indeed, I would like to suggest that Hume’s skepticism with regard to reason at least approximates his inductive skepticism. Just as arguments against his inductive skepticism can be urged, arguments along the lines of Fogelin, Hacking, Imlay, and Karlsson can be urged against his skepticism about reason. But in both cases responses can be made on behalf of Hume; as a result, (unless one finds externalism to be a satisfying response),²² in neither case are the doubts raised by Hume entirely quelled.

NOTES

In writing this, I have benefited from my colleague Noel Fleming as well as from Aaron Boyden, John Collins, Ilhan Inan, and Skip Larkin who attended my Hume seminar. My debt to the referees of *Hume Studies* also extends beyond my expressions of appreciation in the notes.

1 David Hume, *A Treatise of Human Nature*, edited by L. A. Selby-Bigge, 2nd ed. revised by P. H. Nidditch (Oxford: Clarendon Press, 1978).

2 William Edward Morris, “Hume’s Scepticism About Reason,” *Hume Studies* 15.1 (1989): 39-60.

3 Robert A. Imlay, “Hume’s *Of scepticism with regard to reason*: A Study in Contrasting Themes,” *Hume Studies* 7.2 (1981): 124-125.

4 I grant that if I repeatedly insisted that ‘ $55,438+1 = 55,437$ ’ is true under varying optimal conditions of perception, it would be plausible that I attach an aberrant meaning to the terms involved. Add to this that at several points in the calculation I (unwittingly) change the meaning of the words. It would then be possible that each step is correct (according to the meaning assigned at that step) while there is no consistent interpretation of the words which would

make the complex sum right. But this possibility can be dismissed since our usual doubts about complex additions are not that we unwittingly changed the meaning of our words in performing the calculation.

5 Robert Fogelin, *Hume's Skepticism in the Treatise of Human Nature* (London: Routledge and Keegan Paul, 1985), 17.

6 Morris, 51.

7 Fogelin, 17–18.

8 Morris, 52.

9 I am indebted to an anonymous referee of *Hume Studies* for this partial alignment of credibilities with evidence.

10 Ian Hacking, "Hume's Species of Probability," *Philosophical Studies* 33 (1978): 30. That Hacking takes probabilities to be justified degrees of confidence isn't entirely clear. But he does consider the "question of when enough probability makes a proof so that belief, when suitably true, can turn into knowledge" (Hacking, 22).

11 Hacking, 30.

12 For example, let d state that the next car is domestic and i that it is an import. When we shift from 10 to 1,000 observations, Hacking hints that the change might be expressible as going from $[\text{Pr}(d) = .6] \pm .2$ to $[\text{Pr}(d) = .6] \pm .05$. The latter, for example, might be interpreted as: For virtually all classes of cars, the percentage of domestic cars is between 55 percent and 65 percent. We might then say that with 10 cars, the justified degree of confidence $P(d) = .4$ (and $P(i) = .2$) while with 1,000 cars, the justified degree of confidence $P(d) = .55$ (and $P(i) = .35$). Since the justified degrees of confidence $P(d)$ and $P(i)$ don't sum to 1, the degrees of justified confidence $P()$ would not be probabilities. I am indebted to a referee of *Hume Studies* for improvements on this note.

13 Given Hacking's objection, the conditional probabilities I_1 proposes shouldn't be seen as one's revised assessments of o 's probabilities but as a model for measuring one's justified degrees of confidence.

14 Imlay, 126.

15 Ibid.

16 Hans Reichenbach, "Are Phenomenal Reports Absolutely Certain?" *The Philosophical Review* 61 (1952): 152.

17 The one case our tree diagram doesn't represent well is one where o must be false if the reasoning R_1 was mistaken in yielding o 's probability to be x_0 . For example, let o claim the sum S is n . The reasoning based on the "nature of the objects" might be a calculation, and because of the nature of the objects (i.e., sum of numbers), the assigned probability of o is 1. If the calculation was incorrect, S can't be n and the second path leading to o on the diagram drops out because $z = 0$. However, situation represented by the diagram re-emerges when we assess the assessment of the calculation. I have avoided this to avoid the awkwardness of a tree diagram with 8 branches.

18 Mikael Karlsson, "Epistemic Leaks and Epistemic Meltdowns: A Response to William Morris on Scepticism with Regard to Reason," *Hume Studies* 16 (1990): 121-130.

19 This kind of quantity can be generally translated into probabilities: If we let $CR(q)$ be the net credibility and allow negative values for it, $CR(q)$ can generally be taken to be $Cr(q) - Cr(\sim q)$. $CR(q)$ then ranges between -1 and $+1$. Add 1 to avoid negative values of $CR(q)$ and divide by 2 to compress the range from $(0,2)$ to $(0,1)$. In the ship example this would yield $[(.95 - .05) + 1] / 2 = .95$ as the probability of q . This kind of transformation would not convert $Cr(q)$ to probabilities in examples like ours concerning God's existence.

20 *Prima facie*, the product seems to give the most plausible figure for the reduced degree of confidence: If I can have only 80 percent confidence in R_1 and R_1 gives me 90 percent confidence in o , I can claim only 80 percent of the 90 percent confidence R_1 assigns to o .

21 Fogelin, 174.

22 Without committing myself to externalism, I suggested how externalism might escape inductive skepticism (Francis W. Dauer, "Hume's Skeptical Solution and the Causal Theory of Knowledge," *The Philosophical Review* 89 [1980]: 357-378).

Received August 1995

Revised April 1996